# PROCEEDINGS

OF THE

# ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1875–76. No. 95.


NINETY-THIRD SESSION.

*Monday, 6th March* 1876.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

## 1. The Annual Periods of Thunder (with Lightning), Lightning (only), Hail, and Snow, at Oxford. By Mr Buchan.

During the twenty-one years ending 1873, the maximum period of thunder with or without lightning, at Oxford, extended from about 9th April to the end of October, the middle of the period being the first week of July; the three highest days taken consecutively being those immediately preceding the summer solstice. During the five months from November to March, only thirteen cases had occurred during the period. Lightning, on the other hand, had its maximum period from May to November,—particularly during August, September, and October. The maximum period of *hail* was during the first six months of the year; whereas, during the second half of the year, very few cases occurred. The snow period extended from the middle of October to the middle of May,—most falling from December to March,—the absolutely highest month being March.

Thus, *thunder with lightning*, at Oxford, closely follows the sun, the middle of the period being only about ten days after the

summer solstice; *lightning* (only) has its maximum period during that time of the year when the humidity of the air is at its maximum; *hail* is most frequent during that period of the year when the temperature is rising, or when the vertical layers of the atmosphere is in most unstable equilibrium; and *snow* during the coldest months of the year, with this striking peculiarity, that the maximum period is not in the depth of winter, but in March, in the end of winter; immediately after which the curve abruptly falls.

The intimate connection of the thunderstorm with summer rainfall, and the important bearing of the whole four curves on climatology, was referred to.

## 2. Note on the Origin of Thunderstorms. By Prof. Tait.

This Note does not refer so much to those great thunderstorms which extend over hundreds of miles in each direction, as to those small local storms which are often seen of from two to five or six miles only in diameter.

It refers particularly to those which are seen, in summer and autumn, to pass down the Tay valley. They almost invariably come from the westward, and I am told each is almost always accompanied by a storm of similar dimensions passing eastwards down the valley of the Forth. So far as I can ascertain, they seem both to commence almost abruptly somewhere in the district about Ben Ledi and Ben Lomond.

Seen from St Andrews, which they frequently pass at a few miles distance to the northward, they usually appear to be in a state of rotation about a vertical axis. It is not very easy to judge of the relative distances of the various clouds, so as to ascertain the *sense* of the rotation; but, in one case which I observed carefully last autumn, the rotation appeared to be in the *positive* direction,—*i.e.*, opposite to that of the hands of a watch whose face is turned upwards.

If this be generally the case, and if it should be found that the direction of rotation of the companion storm in the Forth valley is *negative*, it would seem that their common origin may be explained on the following very simple hypothesis, which has the

additional recommendation of easily accounting for certain other singular phenomena.

It is known from balloon ascents that, in general, the atmosphere is arranged in horizontal strata of considerable depth or thickness, alternately moist and dry,—temperature diminishing steadily with increase of height in the moist, and remaining nearly constant throughout the dry, strata. These strata have usually horizontal velocities, differing (sometimes considerably) both in magnitude and direction. Thus near the common boundary of two such strata, fluid friction will in general tend to produce vortex motion,—the vortex columns being at first nearly horizontal, with their ends at the boundary, which is a surface of discontinuity.

A complete investigation of the possible circumstances would show *four* quite different cases:—

Vortex formed in $\left\{\begin{matrix}\text{dry}\\ \text{moist}\end{matrix}\right\}$ air, with its ends turning $\left\{\begin{matrix}\text{down}\\ \text{up}\end{matrix}\right\}$ into a stratum of $\left\{\begin{matrix}\text{moist}\\ \text{dry}\end{matrix}\right\}$ air.

The half vortex-ring thus formed tends, so far as it can, to become semicircular. It may thus extend downwards to the earth or upwards into the higher regions of the atmosphere.

If it extend downwards nearly to the earth, the lower portion will soon be destroyed by friction, and we shall have a couple of vertical vortex columns, with their ends respectively in the surface of discontinuity, and on the ground. They will of course rotate in opposite directions about the vertical, and their mutual influence will tend to cause them to progress in directions parallel to one another, the motion of each being in the same direction as that of the rotatory motion of the side which it at the moment turns to the other. This is exactly the presumed case of the little storms in the Tay and Forth valleys above referred to; the south side of the Tay column (that turned towards the Forth), moving eastward about the axis, while the axis itself moves to the east.

This theory is evidently capable of at once explaining the apparently *sudden* occurrence of such storms (of which waterspouts must be looked upon as small but quickly rotating examples), when the lower atmosphere has for hours been in a dead calm.

The disturbance has, in fact, its origin *above* the lower stratum, and works its way downwards into it.

It is also competent to explain the production of similar rotating storms in the higher regions of the atmosphere—many miles above the earth's surface—and thus to account for that by no means small number of cases of so-called "summer-lightning," which obviously cannot be explained by the occurrence of an ordinary thunderstorm at such a distance as to be below the spectator's horizon.

I have already explained to the Society that a possible source of at least a large part of the electric charge of a thunder-cloud is the contact-electricity of water-vapour and air. Thus while the precipitation of the vapour develops heat, the water particles precipitated are strongly electrified. And the aggregation into one of a number of equal little drops all charged to the same potential may increase the potential in any ratio whatever. Thus the charge on each drop in a large cloud may become so great that the electricity is driven entirely to the particles at its surface. This is supplementary to, and does not interfere with, Sir W. Thomson's explanation of the process by which the vapour is condensed.

It is possible that taking place in greatly larger spaces of air, but to a much smaller extent in each cubic foot, this sudden production, and as sudden scattering in all directions, of considerable quantities of electricity, may account for some of the main phenomena of the Aurora.

## 3. An Application of Professor James Thomson's Integrator to harmonic Analyses of Meteorological, Tidal, and other Phenomena, and to the Integration of Differential Equations. By Sir W. Thomson.

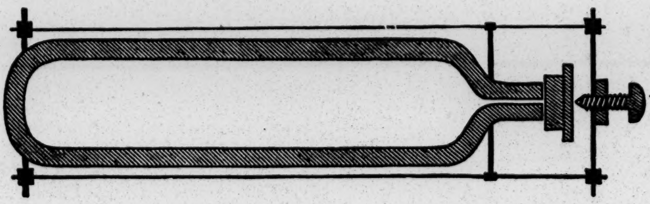
A first rough Model of Professor J. Thomson's Integrator was shown.

## 4. Note on the Thermo-Electric Position of Cobalt. By Professor Tait.

5. On a Glass Digester in which to Heat Substances under Pressure. By Dr E. A. Letts.

The objections to the use of sealed tubes are known to every practical chemist, and are a serious drawback to their employment. The chief of these are the time expended in the manufacture of the tubes, the amount of skill in glass-blowing required, the danger experienced in opening them, and above all, the fact that only a small quantity of material can be heated at one operation. Moreover, the same tube can seldom be used for more than three or four experiments, as each time it is sealed up its neck must be drawn out, and its length thus considerably decreased. These disadvantages were especially felt by me whilst preparing bromacetic acid, which was required in considerable quantities, and where as many as half a dozen tubes of bromine and acetic acid had to be heated before 100 grammes of the acid could be obtained. To obviate these objections I have had an apparatus constructed, which consists of a cylinder of glass, the walls of which are about half an inch thick. Its length is fifteen inches, its external diameter three inches, and its capacity about 600 cubic centimetres. At one end it is drawn out to a tube, whose aperture is only about one-sixth of an inch in diameter, though its walls are as thick as the rest of the apparatus. Originally this tube was provided with a stopcock, but at Professor Brown's suggestion, I have substituted a glass plate, which is ground flat, and accurately adapted to the top of the tube.

In order to keep the glass plate pressing on the tube the whole apparatus is placed in a frame, consisting of three brass wires arranged symmetrically around the cylinder, and attached by means of nuts, below, to a brass ring, and above, to a brass plate, through which latter a screw passes, which, when turned, presses on a brass plate placed on the glass cap.



As any experiments with such an apparatus would be attended

with danger, were it necessary to be in its neighbourhood, it occurred to me that an automatic arrangement might be employed to give notice that the temperature had been reached to which it was intended to subject the digester.

For this purpose I employed a thermometer with a somewhat wide tube and large bulb. A platinum wire is sealed into the bulb, and touches the mercury, whilst a brass wire passes down the tube, and is held in position by a binding screw. The two wires are connected with an electric bell, the brass wire being so adjusted, that when a particular temperature is reached the mercury touches its end, and thus completes the circuit, and causes the bell to ring.

In order to test the digester, about 200 grammes of a mixture of two-parts bromine and three of acetic acid was placed in it, and after fixing it in its frame, the whole apparatus was immersed in an oil bath and heated to 150° C., the temperature at which reaction in this case takes place. The experiment was made in a cellar, and the bell placed in a room some distance off. The gas to heat the oil bath was led by a tube from another cellar, so that it could be regulated without going near the digester. In about an hour and a half the bell rung, and thereupon the gas was shut off; and on examining the digester next day, it was found that the reaction had taken place, and that only twelve grammes of product had been lost—a very inconsiderable quantity.

As the action of bromine on acetic acid is very sudden, and accompanied by the disengagement of a large volume of hydrobromic acid, the apparatus may be considered to have undergone a very severe test, and that its efficacy for all ordinary purposes is established.

Should the digester come into general use, it will certainly save chemists much time and labour.

The following Gentlemen were elected Fellows of the Society:—

Rev. Francis Le Grix White, M.A.
James Duncan, Esq., Benmore.
Rev. Norman Macleod.
J. S. Fleming, Esq.
James Douglas H. Dickson, M.A. Glasg., B.A. Camb.

*Monday, 20th March* 1876.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

### 1. On the Connection between Cohesion, Elasticity, Dilatation, and Temperature. By Professor George Forbes.

(*Abstract.*)

At various times there have arisen supporters of one or other of two extreme hypothesis concerning the nature of what we define as force. These are the hypothesis of "action at a distance" and of "no action at a distance."

According to the latter hypothesis, the centre of gravity of no body, however large or however small, can be moved from a position of rest, nor can its motion be altered in direction or amount, except by direct collision with another portion of matter.

Starting from this supposition as a basis of argument, and without assuming anything further as to the manner in which the different physical forces are caused by collisions, it is possible to arrive at some very general theorems; and from these theorems conclusions may be drawn as the nature and connection of some of the physical forces, which are necessarily true if the hypothesis of no action at a distance be true.

The principal result of these theorems is the following:—Let a rod be chosen of any substance whose cohesion and elasticity do not vary enormously with the temperature. Let $a$ be its expansion, in terms of its length, when the temperature is raised 1°. Let $\beta$ be the compression of the same rod, in terms of its length, when a unit weight is supported at its summit. Let $c$ be the number of these units which, when suspended by the rod, suffice to break it by sudden rupture. Let $\theta$ be the absolute temperature at which all these experiments are made. Then the theory leads us to the conclusion that

$$a = \frac{\beta c}{\theta}.$$

Only a few experiments have been made by which we can test

this law. But the following values are the most accurate, and tend to prove the truth of the law. The apparent discordance in the case of iron is in part due to the variations in the qualities of that metal in different specimens.

| | $\frac{\beta c}{\theta}$ | $a$ |
|---|---|---|
| Gold, . . . . . | ·00001484 | ·00001358 |
| Silver, . . . . | ·00001796 | ·00001809 |
| Copper, . . . . | ·00001511 | ·00001481 |
| Platinum, . . . | ·00001006 | ·00000851 |
| Iron, . . . . . | ·00001573 | ·00001220 |

In calculating this table, the values of $c$ from the experiments of M. Wersheim are used; those of $a$ from the experiments of Mr Mathiessen (except iron); those of $\beta$ from the experiments quoted by Prof. Balfour Stewart in his Text-Book; and the assumed temperature is 18° C., or 283° absolute temperature.

## 2. Notice of the Completion of the Works designed by Sir Charles A. Hartley, F.R.S.E., for the Improvement of the Danube. By David Stevenson, Esq., V.P.R.S.E.

In 1868 I presented to the Society, on behalf of Sir Charles A. Hartley, a memoir published by the European Commission of the Danube, on the improvement of that river, and at the same time gave a notice of the works designed by Sir Charles Hartley for effecting that important object. These works have now been completed, and Sir Charles Hartley has again asked me to present to the Society a second memoir published by the Commission, which brings the history of the works constructed under their charge down to the time of their completion in 1873.

In supplement of the notice formerly communicated, which referred to a work in progress, it may not be uninteresting, now that the work is completed, to state briefly what has been effected by this most important and successful example of hydraulic engineering.

The engineering problem to be solved by the European Commission was the removal of the bar which obstructed the Sulina mouth of the Danube, which, in 1856, had a varying depth of channel never exceeding 11 feet. The design of Sir Charles Hartley—the engineer to the Commission—consisted in piers so constructed as to confine the current of the river in its passage into the Black Sea. At the date of my last notice the north pier had been extended to the length of 4640 feet, and the south pier to 3000 feet, and a maximum depth of 17½ feet instead of 11 feet had been obtained. I, however, suggested in that notice, that as the Danube must continue to bring down an enormous mass of detritus, so in course of time the works which had proved so successful must be extended; and it appears that this has been found necessary, as the south breakwater, completed in 1871, has been extended to 3457 feet in length, and even with this additional length it is, I think, not improbable that in the course of time still farther extension may be required, for the Sulina mouth of the Danube will still discharge the same amount of water, bearing with it the same amount of alluvial matter, estimated in high floods at about 70,000 tons in twenty-four hours, the deposit of which at the extremity of the piers will still have a tendency, though in deeper water, to form a bar.

The works have, however, proved most successful, and reflect the highest credit on Sir Charles Hartley, by whom they were designed and executed, and the following is a summary of the results that have been obtained.

The total length of piers executed is 8789 feet, at a cost of L.185,352, being L.21 per lineal foot, in an average depth of 14 feet at low water. The navigable depth of the channel over the bar has been increased from 11 feet in 1856, to 20 feet in 1873. In 1853, 2490 vessels, of 339,457 aggregate tonnage, left the port; in 1869 there were 2881 vessels, with a tonnage of 676,960. Thus, while the number of vessels increased only at the rate of 16 per cent., the tonnage, due to the greater draught, had been increased at the rate of 50 per cent., a good practical proof of the value of the improvements. The number of shipwrecks at the mouth of the Danube has also been greatly diminished.

*Monday, 3d April* 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Chapters on the Mineralogy of Scotland. By Professor Heddle. Chapter I.—On the Rhombohedral Carbonates. Communicated by Professor Tait.

Professor Heddle read a paper on the "Rhombohedral Carbonates occurring in Scotland," the first of a series of Chapters intended to embrace the analytical results of an investigation of all unknown or insufficiently determined Scottish species.

In this paper many analyses of the carbonates were submitted; and the pseudomorphic changes taking place in these were referred to in a special manner.

2. On Thermo-Dynamic Motivity. By Sir W. Thomson.

3. On the Vortex Theory of Gases, of the Condensation of Gases on Solids, and of the Continuity between the Gaseous and Liquid State of Matter. By Sir W. Thomson.

4. On two new Laboratory Apparatus. By William Dittmar.

The object of this communication is to submit to the notice of the Society two little inventions of mine, which, whatever may be the degree of originality which they can claim, will, I venture to hope, prove useful additions to the catalogue of chemical-laboratory appliances. The one is a new form of the precision balance, which pretends to execute *exact* weighings with a hitherto unattained degree of rapidity; the other is a contrivance for maintaining a constant pressure in a supply of gas, and thus making it possible, with comparative facility, to keep, say an air-bath, for any length of time, at a constant temperature.

The new *balance* differs from the instrument in its customary

form only in two points, of which the more important is a modification of the centre of gravity "bob" arrangement, which enables one, at a moment's notice, to shift the centre of gravity of the instrument from a certain definite position, I., to a certain other (higher) position, II., matters being arranged so that in passing from I. to II., the sensibility, *i.e.* the deviation, corresponding to an overweight of, say 1 milligramme, is increased in an exactly pre-determined ratio, such as of 1 : 10, for instance. For this purpose the "bob" is made very light, so that the distance through which it has to travel in order to effect the desired change of sensibility is not too small, and, instead of to a screw as usual, is fixed by *mere friction* to a vertical triangular steel rod forming part of the needle. The other new feature in the balance is, that the rider-principle, besides being discounted in a slightly different manner from the customary one, is extended to the determination of differences of weight up to 100 (instead of 10) milligrammes.

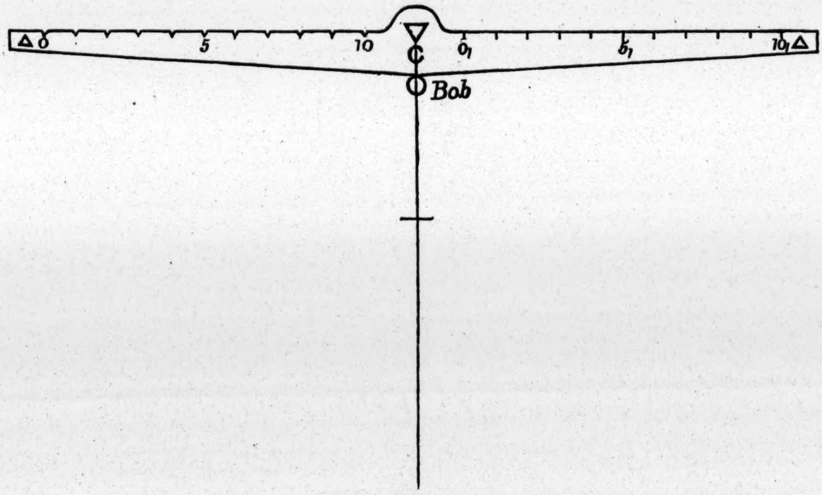



Fig. 1.

The arrangement adopted is represented in the accompanying sketch, for the interpretation of which it is only necessary to say that C (10) and (10) O are equal to C $(0)_1$ and $(O)_1$ $(10)_1$ respectively, and that both O (10) and $O_1$ $(10)_1$ are each divided into 10 equal parts, the former by *notches* filed into the beam, the latter by *marks;* and to add, that there are two riders, one weighing $p$ centigrammes for the left arm, and another weighing $p$ milligrammes

for the right arm, the balance being adjusted so that, when both riders are at their zero-points, it is in equilibrium, and $p$ being chosen so, that, supposing the large rider to be shifted to the $n$ mark, and the small one to the $m$ mark, this virtually amounts to the addition of $10n+m$ milligrammes to the charge in the right pan.

There is no need of my explaining how the balance is meant to be used; I will rather avail myself of this opportunity for drawing the attention of readers interested in the subject to a few inferences from the theory of the balance, which, obvious as they are, have hitherto not been sufficiently appreciated by either the authors of our physical handbooks or by practical balance-makers.

I. Given a balance in which everything is constant except the distance $s$ of the centre of gravity of the empty instrument from the axis of rotation, and it is easily shown that (for a constant charge) the deviation $\alpha$ of the needle for a given over-weight $\Delta$, and consequently the "sensibility" $a = \frac{\alpha}{\Delta}$, is the greater the less $s$. This, of course, is duly stated by all authors; but what is always forgotten to be pointed out are two things, viz.—1st, That the "*sensibility*" has nothing to do with the *inherent precision* of the instrument; and 2dly, That supposing the sensibility to be increased, all the other good qualities of the balance get *less;* we diminish the rate of vibration (this rate being proportional to $\sqrt{\frac{1}{a}}$); we diminish the range of differences of weight determinable by the method of vibration; we diminish the relative constancy (in opposition to variations in the charge) of the sensibility and the time of vibration. Considering now,—

II. The case of a *balance to be constructed*, the arm-length $l$ and weight $w$ of the empty beam also become variables, related to each other, according to some equation like $w =$ const. $l$, and (assuming each of the pans to bear a certain medium charge $p$) we have

$$\frac{t}{\sqrt{a}} = \text{const. } \sqrt{l}\ \sqrt{\text{const.} + \text{const. } l},$$

*i.e.*, by diminishing $l$ we can increase the sensibility without diminishing the rate of vibration (or *vice versâ*); but the other

disadvantages mentioned under I. must be taken into the bargain, and, besides, the inherent precision of the balance gets less.* To pass to an example: What we gain by substituting a 7-inch for a 14 inch beam is that, for the *most convenient t*, the sensibility becomes 2 to 4 times greater; but this advantage is secured without expense in good qualities by placing before the graduated limb a lens magnifying the excursions of the needle into 2 to 4 times their natural size. This is the theory of the "*short beams*," which have lately come so much into fashion.

To come back to my own balance, I must not forget to thank Messrs Becker Sons of Rotterdam for the readiness with which they have, at their own risk, tried to realise my ideas in an actual instrument, which, by the way, is now being exhibited at South Kensington. To increase the usefulness of the instrument, I have caused Messrs Becker to add to it a glass plunger, which is adjusted so that it displaces exactly 10 grammes of water at 15°, and which consequently enables one with great rapidity to determine the specific gravities of liquids by the method of immersion.

To pass now to the new *gas-governor*, its most essential part consists of a mercury-manometer (fig. 2), of which one limb, A, is

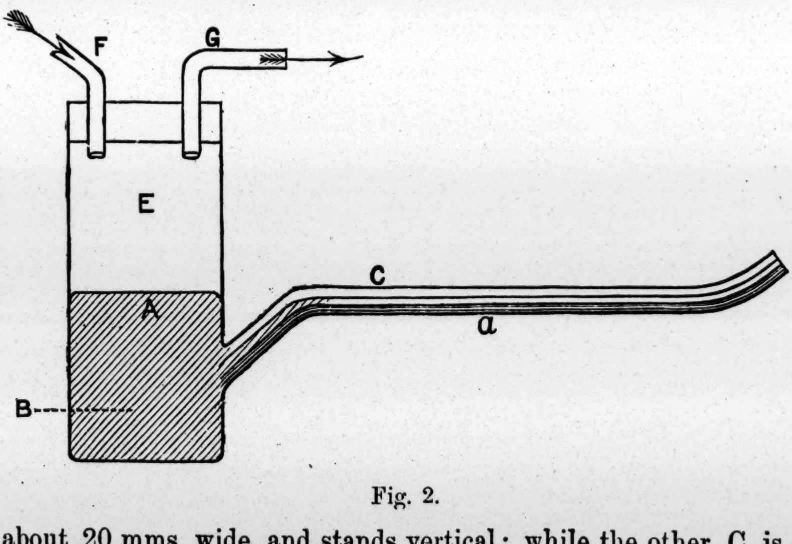



Fig. 2.

about 20 mms. wide, and stands vertical; while the other, C, is of the width of a thermometer tube, and is placed horizontally.

* For fuller explanations, see my article "Balance" in the "Encyclopædia Britannica."

The empty part of the wider limb communicates, through F, with the gas-supply, through G with the gas-lamp serving to heat the air-bath; and the quantity of mercury is adjusted so that, when the gas is at the lowest pressure which, in the course of the experiment, it is likely to assume, the mercurial index in C occupies a certain convenient position *a*.

The manometer is connected with a constant battery (the circuit of which includes an electro-magnetic arrangement for opening or shutting the gas-tap), in such a manner that, as soon as, through an increase of pressure in E, the index in C travels ever so little towards the right of *a*, the current is closed, and the gas cut off.

The following Gentlemen were elected Fellows of the Society:—

John Macmillan, M.A.
John Gibson Cazenove, D.D.

The following Gentlemen were elected Honorary Fellows of the Society:—

1. *Foreign Honorary.*

Carl Ludwig, Leipzig.
Ferdinand de Lesseps, Paris.

2. *British Honorary.*

Henry John Stephen Smith, Oxford.
Thomas Henry Huxley, London.
Thomas Romney Robinson, D.D., Armagh.

*Monday, 17th April* 1876.

Professor FLEEMING JENKIN in the Chair.

The following Communications were read:—

## 1. On an Improved Form of Galvanic Battery. By J. Cook, Esq. Communicated by Professor Tait.

I wish to direct attention to a simple improvement on battery cells, whereby porous cells are dispensed with, and the inconveniences of gravity batteries avoided.

I may say it is a year or two since I first tried the plan.

It consists in first filling the outer glass cell one-third or one-half with fine *silver* sand, then pushing a ring of glass (which I cut from a common ale pint-bottle with a hot soldering bolt) down an inch or so into the sand. The zinc element forms a ring round the glass, and the copper lies as a plate on the sand within the glass. Its superiority to the gravity batteries, and to those, such as Sir William Thomson's, where the sand forms a dividing layer between the copper with its sulphate below and the zinc with its liquid above, will be obvious. I did not find the cupric sulphate solution to diffuse into the zinc division. It so readily admits of inspection that it would be infinitely preferable to the Meidinger and other plans.

## 2. On the Properties of the Perigon Versor. By G. J. P. Grieve, Esq. Communicated by Professor Kelland.

## 3. Descriptions of some new or imperfectly understood Forms of Palæozoic Corals. By H. Alleyne Nicholson, M.D., D.Sc., F.R.S.E., Professor of Natural History in the University of St Andrews, and James Thomson, F.G.S.

(*Abstract*).

In this communication the authors gave descriptions of several new or imperfectly understood forms of Palæozoic corals. After giving a general account of the method of investigation employed by them, the genus *Heliophyllum*, Hall, was discussed at length. The external structure of this genus is very peculiar, and it was shown that the genus is not by any means as nearly related to *Cyathophyllum* as has been generally believed. The new genus *Crepidophyllum* was proposed for a group of forms possessing the extraordinary and characteristic endothecal dissepiments of *Heliophyllum*, but with the remarkable character that the central tabulate area of the corallum is shut in by a well-developed accessory wall or inner mural investment. Sometimes this secondary investment constitutes a complete circular sheath to the central tabulate area, and in this case all the primary septa become

directly connected with the outer surface of the cylindrical tube thus formed. More commonly, the secondary investment is open all down one side, and becomes directly continuous with two of the primary septa, thus constituting a horse-shoe shaped space, formed by the central tabulate area together with a wide fossula containing three short septa. It was shown that the fine coral described by Mr Billings under the name of *Diphyphyllum Archiaci* was truly a *Crepidophyllum.* It was further shown that two different forms, of very similar aspect, had been included by one of the authors under the name of *Heliophyllum sub-cæspitosum.* One of these forms, the typical one, is a *Crepidophyllum,* and will stand as *C. sub-cæspitosum.* The other is a *Heliophyllum,* and the authors described this under the name of *H. elegantulum.*

The name of *Thysanophyllum* was proposed for a genus of æstræiform corals from the Carboniferous rocks of Scotland. This genus is related to *Lonsdaleia* in the general form of the corallum, in the presence of an exterior vesicular zone of large-sized cells, and in the possession of septa, which have no connection with the outer wall. It differs from *Lonsdaleia,* however, in the fact that the columella, so conspicuous in the latter genus, is wholly wanting, and the central area of the visceral chamber is occupied by strong remote, transverse tabulæ. Two species of the genus were described, under the names of *Thysanophyllum orientale* and *T. minus.*

Finally, the genus *Lindströmia* was proposed for a group of small corals, in which the corallum is simple and conical, with an extremely deep calice. The septa are well developed, and meet in the centre of the visceral chamber, where they coalesce to a greater or less extent, and form a strong twisted pseudo-columella, which projects into the floor of the calice, and occupies a large portion of the entire visceral chamber. There are no tabulæ, but the septa are furnished with more or less strongly developed dissepiments, which, however, are remote, and do not give rise to any vesicular zone. The genus may, perhaps, be regarded as belonging to the *Aporosa.* The species *L. columnaris* was described from the Devonian rocks of North America, and it was mentioned that the authors were in possession of other forms of the genus, still undescribed, from the Carboniferous rocks of Scotland.

4. On a Stable and Flexible Arch. By Professor Fleeming Jenkin.

*Monday*, 1*st May* 1876.

Professor KELLAND, Vice-President, in the Chair.

The Council have awarded the Keith Prize for the biennial period 1873–75, to Professor CRUM BROWN, for his Researches on the Sense of Rotation, and on the Anatomical Relations of the Semi-circular Canals of the Internal Ear.

The following Communication was read:—

Is the Gaelic Ossian a Translation from the English? By Professor Blackie.

The recent revival by a distinguished Celtic scholar of the theory of Laing that Macpherson's Gaelic Ossian is a translation from the English, affords an opportunity of examining that question in a more strictly philological fashion than it has hitherto had the fortune to enjoy. Parts of the question were no doubt touched by Mackenzie in the Report of the Highland Society, published in 1805 by Graham in his dissertation on the authenticity of Ossian, by Dr Clerk of Kilmalie, the distinguished author of the new version of Ossian in the late splendid edition published at the expense of the Marquis of Bute; but systematically grappled with the question has never been. Having recently gone through the whole of the originals, I have made careful notes of whatever might tend to settle this question, and have come to the conclusion, in the face of the statement of Mr Campbell—whose authority, no doubt, is one of the highest on the subject, that the Gaelic is unquestionably the original. The tests by which a translator's hand seems clearly discoverable are the following five:—(1) In the English version, awkward, forced, and unidiomatic expressions frequently occur, which can be clearly traced to the influence of a Gaelic original. (2) In all poems of any antiquity handed down

in manuscripts, difficulties will occur arising from obsolete words, errors in transcription, confused connection, and other causes. In such cases it is a common practice with translators to skip the difficulty, gloss over the matter with some decent commonplace, and sometime to make positive blunders, which is not difficult for a philologer to expose. All these signs of a translator's hand are frequent in Macpherson's English, and would be more so had he not indulged in such a habit of skipping generally that it is difficult to say in certain cases decidedly that the skip was made because the writer of the English wished to shirk a difficulty. (3) It is a common practice with translators, when they find a passage a little obscure, to remove the obscurity by some manifest alteration of the phrase, or even by interpolating a line, or interlarding a commentary. This also occurs in Macpherson. (4) It is not always that a translator writes under the same vivid vision, or the same fervid inspiration as the original poet; and the consequence is that he will occasionally degrade poetry into prose, and specially fail to bring out that individuality of character in his word-painting which Ruskin has so triumphantly insisted on in the case of the sister art. The instances of failure to seize the most striking features of the original, and the substitution of generic for specific epithets, are frequent in Macpherson. (5) Most translators yie —sometimes, no doubt, wisely—to the temptation of improving on their originals; and Macpherson, from what we know of him, was the last man in the world to think of resisting such a temptation. How much of the Gaelic, as we now have it—that is, his clean copy of his own originals—was subjected to this process of beautification, as we may call it, no one can now tell, but I have traced in several instances departures from the simplicity of the original Gaelic, which can be explained most naturally on the supposition that they proceed from a translator who has yielded, without any just cause, to this flattering seduction. When the results obtained by the detailed application of these tests are combined with the amount of external evidence to be found in the Highland Society's report to the effect that Macpherson actually did translate from Gaelic originals, and was seen by various parties for weeks and months employed in the work of traslation, a cumulative proof was produced that he was most anxious to see by what arguments Mr

Campbell could rebut. If that gentleman, to whom Celtic literature owes so much (and who in fact is the Wolf of the Ossianic question), or any Galician who thinks with him, shall succeed in leading a counterproof, I can only conclude that, considering the scrappy and fragmentary nature of some of the materials in Macpherson's hands, it might possibly have been the case that the translator filled up some of the gaps in his tale in English, with the intention that they might be done into Gaelic before publication by Strathmaskie, Captain Morrison, or some other of his Highland coadjutors; but that the English, as a whole, is a translation from the Gaelic, and not a translation of the best quality in many respects, may be accepted as one of the best ascertained facts in the whole range of philological investigation.

The following Gentlemen were elected Fellows of the Society:—

Professor M. Forster Heddle.
J. F. Rodger, S.S.C.
William Thomson, F.C.S., Manchester.

*Monday, 15th May* 1876.

Sir WILLIAM THOMSON, President, in the Chair.

The Keith Prize for the biennial period 1873–75, which has been awarded to Professor Crum Brown for his Researches on the Sense of Rotation, and on the Anatomical Relations of the Semi-circular Canals of the Internal Ear, was presented by the President.

The following Communications were read:—

1. Notes on Dredging in Madeira, by the Rev. Robert Boog Watson, B.A., F.R.S.E., F.G.S.

The difficulties in the way of dredging at Madeira are many and considerable. This has probably prevented any of this work having been done since the publication of Mr Macandrew's list of Mollusca, presented to the British Association in 1854. The author

having dredged for several years at Madeira, confirms Macandrew's generalisation of the Mediterranean character of the Mollusca—excludes 12 of Macandrew's named list as having crept in by mistake, and to the 115 remaining species identified by Macandrew as Madeiran has added 200 to 250 more, making nearly 400 in all, of which 80 or perhaps 100 are probably new. These he hopes soon to publish.

## 2. Note on a New Fossil Foot-Print from the Permian Sandstone of Dumfriesshire. By Patrick Dudgeon, Esq., F.R.S.E. (Plate I.)

What appears to be an entirely new foot-print has lately been found in the red sandstone of this district. I have seen many of the foot-prints from the various quarries in the neighbourhood, but have not before observed this one, nor is it like any figured in Sir William Jardine's splendid work on the "Ichnology of Annandale." The foot-prints in question were found in a bed about 20 feet from the surface, at Locharbrigg's Quarry, three miles from Dumfries. They exhibit the usual large hind and smaller fore foot; the impression of the hind foot measures ′·5 × 2′·6, the fore foot 2′·3 × 1′·9; the stride of the animal appears to have been about 10′. The impression of the hind foot does not interfere with that of the fore foot, as is the case with several of the foot-prints figured in Sir William Jardine's work, the interval between them being 2′: the hind foot, therefore, must have been put down in the rear of the fore foot when the animal was walking. The impression of the foot shows five toes, the *thumb* being placed far back. The most characteristic features in these foot-prints are the well-developed claws, and the oblique position of the toes, *i.e.*, they are placed to *march* one behind the other. In almost all the foot-prints I have seen, where the toes can be made out, the middle one appears the most prominent; this foot-print is markedly distinct in this respect.

As yet I have only been able to obtain one good specimen of this foot-print—a hind foot; the rest of the slab on which the casts were impressed was unfortunately used for a paving stone in a cottage in the neighbourhood. I got it lifted; but the rough

Mc Farlane & Erskine. Lithrs Edinr.

usage it had been subjected to had greatly injured the impressions on it; they were, however, sufficiently distinct to enable me to give the above particulars.

The accompanying photograph is a good one of the hind foot in my possession, about half the size of the original. The posterior pad of the foot is not quite complete, and it, together with the pads of the toes, are somewhat broken.

I would propose for these foot-prints the provisional name of *Herpetichnus loxodactylus*, the oblique-toed Herpetichnus, with the following abbreviated character:—

GENUS HERPETICHNUS, *Jardine* ("Ichnology of Annandale," 1853, p. 14).

*Herpetichnus loxodactylus*, sp. nov.

*Sp. chars.*—Fore foot = 2′·3 × 1′·9; hind foot = 3′·5 × 1′·6; stride about 10′; impressions free; toes 5, oblique; thumb far back; claws well developed.

*Locality and horizon.*—Permian Sandstone, Locharbriggs Quarry, three miles from Dumfries.

*P.S.*—In the discussion which followed this paper, Professor Huxley stated that so far as he could judge from the photograph exhibited, the markings closely resembled a foot-print he had described some years before in a paper read before the Geological Society of London, "On the *Stagonolepis Robertsoni* (Agassiz) of the Elgin Sandstones; and on the recently discovered Foot-marks in the Sandstone of Cummingstone" ("Quart. Jour. Geol. Soc.," 1859, xv. p. 440). The resemblance of these Cummingstone foot-marks to the *Chelichnus* of the Dumfriesshire flags was noticed by Professor Huxley in the paper referred to.

## 3. On the Decennial Period in the Mean Amplitude of the Diurnal Oscillation and Disturbance of the Magnetic Needle and of the Sun-spot Area. By J. A. Broun, F.R.S.

(*Abstract.*)

The author, in presenting results relating to the decennial period derived from observations made at Trevandrum during twenty-two years, has sought a redetermination of the mean duration of that

period, as shown by preceding magnetical observations connected with his own. The relation of the frequency and area of sun-spots to the amplitude of the diurnal movements of the magnetic needle gives an increased value to this investigation.

Two very different results have been obtained;—one by Dr Lamont, showing a period of 10·4 years; the other, by Dr R. Wolf, gives $11\frac{1}{9}$ years. Dr Lamont's result depends on the assumption that three periods occurred between 1787 and 1818—an assumption which is opposed to the conclusions which have been deduced from the sun-spot, auroral, and magnetic observations for that interval. Dr Wolf's result has therefore been accepted very generally by many of the most eminent scientific men in England and on the Continent.

The author determines the epochs of maximum and minimum range of the diurnal oscillations of the magnetic needle by the more exact method, in which the mean for twelve months corresponding (at its middle point) to each month of the year is obtained. Commencing with the Trevandrum observations, from the present time, proceeding backwards to the earliest series, showing a maximum, that of Cassini (Paris 1784–1788.) The maximum at this time (1787·25) is confirmed nearly by Gilpin's observations (London, 1786–1806). The latter do not show the minimum in 1792 and maximum in 1797, which should satisfy Dr Lamont's assumption, and they are considered by him, like the observations of sun-spots at the time, as worthless for this investigation. Dr Wolf, on the other hand, finds support in both for a minimum in 1798.

It is concluded by the author, from an examination of Gilpin's observations, that a maximum really happened in 1797·7, but so little marked as to make it probable that any slight corresponding increase of sun-spots would not be noticed by the single, not very accurate, observer at the time. Evidence, however, of a slight maximum is also found in Professor Loomis's investigation for the frequency of the aurora borealis. As it is certain that another maximum occurred about 1804 to 1806, the author finds that Gilpin's observations, which agreed with Cassini's at the commencement of the series, showed in all probability the true magnetic variations afterwards.

It results from these investigations that the mean duration of the

period is 10·45 years; but that it appears to undergo a variation between 8 and 12½ years in an interval of 42 years. The small maximum of 1797·7, if a true result, may be expected to repeat itself at some future time, a result which could not fail to aid in the search for the cause of these variations.

The author shows, that according to the long period of 42 years, a *maximum* should have happened in 1776; but that year Dr Wolf has concluded to be one of *minimum* sun-spot frequency. That 1776 was really a year of maximum is confirmed by the observations of Van Swinden, who, it is shown, appears to have been the first to obtain a variation due to the decennial period, and to have pointed out the appearance of a law: it is also confirmed by the observations of Cotté at Montmerency.

The ratio of the ranges of the diurnal variation in the years when it is a maximum to that in the years of minimum, is compared for different parts of the world, and found nearly the same in both hemispheres. It is also found that the law of the diurnal variation is the same in the year of maximum as in the year of minimum. The author concludes that the increase of the diurnal variation is not due to a different cause from that which produces the variation at the minimum, and that this cause acts when there are no sun-spots in the same way as, though with less intensity than, when the latter have their maximum frequency and area. The magnetic variations are therefore not due to the sun-spots; the latter appearing only when the common cause produces diurnal variations having at least two-thirds of the maximum amplitude.

The results derived from the sun-spot area are compared with those from the magnetic observations. While a general agreement is found in the decennial variations from year to year, it is evident that the attempt to calculate the amplitude of the diurnal variation from the sun-spot frequency (as has been done by Dr Wolf) must give results frequently deviating widely from the truth, as might be expected from the previous conclusion.

The decennial period of disturbance of magnetic declination at Trevandrum, deduced from hourly observations in the eleven years, 1854 to 1864, is next considered. It is found that the mean disturbance at each hour of the day shows the decennial period; but that the range of the mean value, from the minimum to the maximum

year, is different for each hour, while the maximum and minimum do not happen at exactly the same time for all hours of the day. Secondary maxima and minima are also shown, which vary in their epochs gradually from midnight to noon.

No clear law appears to connect the amount of the maximum disturbance for any hour with that of the minimum for the same hour in the 11 years; the ratio of the first to the second is least for the hours near noon, and greatest for those near midnight. It is found, however, that the maximum and minimum mean disturbance in the diurnal variation for each year, as well as in the decennial variation for each hour, are connected by the following relations:—$D_m$ being the maximum and $D_o$ the minimum disturbance.

$$\sqrt{D_m} - \sqrt{D_o} = \text{Constant.}$$

The *monthly* mean disturbance at Trevandrum in each of the years 1854 to 1864 is compared with the monthly mean sun-spot areas deduced by Messrs De La Rue, Stewart, and Lœwy, from Carrington's and the Kew Observations, with the following result:—The monthly mean disturbance in the years 1854–56 had a considerable value, and marked variations when there were few or no sun-spots. In 1857 to 1862 there are found several maxima and minima of disturbance and sun-spots which occur simultaneously. In some cases, and especially in June 1862, there is a well-marked sun-spot area maximum without any corresponding change of magnetic disturbance. The cause of the solar disturbance did not then extend its action to the earth at that time.

The author concludes with a notice of the hypotheses proposed to explain the decennial period of magnetic variations and of sun-spot frequency, as well as of the cause of sun-spots. It is pointed out that no theory of sun-spot formation can be accepted which does not explain their non- (or very rare) appearance every 10 or 11 years, and therefore the cause of the decennial period is bound up with this explanation. A planetary action which disturbs the equilibrium of the solar gases has been proposed; no other seems to present itself, and this the author believes will be found ultimately to be in question; and though he has not himself been able to find any satisfactory evidence in its favour, yet remarkable results have been obtained by Messrs De De Rue, Stewart, and Lœwy.

## 4. On the Parallel Roads of Lochaber. By David Milne Home, LL.D.

*(Abstract.)*

The author referred to the papers written on the subject, beginning with that by Dr Macculloch, in the year 1817; and he explained the various theories suggested.

He intimated his adoption of the Lake theory, and expressed his adherence to the view he took in the memoir read by him in this Society in the year 1846, that the blockages of the lakes had been effected by detrital matter.

In support of this view, he pointed out that all over this district of the Highlands there were immense beds of clay, sand, and gravel up to the tops of the hills, at even 2000 feet above the present level of the sea.

These deposits he considered to be undeniable proofs of the prevalence of the sea over this part of the earth's surface to a height of 3000 feet at least.

When the sea retired, so as to expose to atmospheric action the higher parts of the country, there would be depressions in the surface of the land, where lakes would be formed. These lakes would continue at high levels, till the streams issuing from them cut through the detritus. In some cases, the process of erosion would be so gradual, that the lakes would subside without producing any conspicuous beach-lines on the mountain sides. In other cases, the removal of the blockages would be on a large scale, owing to the looseness of the detritus; and if these removals were separated from one another by a considerable interval of time, beach-lines of a permanent character would be formed on the sides of the mountains enclosing the valleys.

The author referred to the existence in this district of the Highlands, now, of several lakes at high levels, which were kept up by detrital blockages. He instanced, in particular, Loch Earba, in the Lochaber district, at a height of about 1150 feet, which was kept up by such means, and on whose banks there was evidence that the lake had once stood 30 feet higher than at present. Near Kingussie there was Loch Gwynae, at a height of 1015 feet above the sea, on whose sides there were traces of five terraces, the highest of which is 132 feet above the present surface of the lake.

He referred to the ample means of cutting through and removing detrital matter possessed by streams and rivers, mentioning particularly the enormous cliffs of detritus cut through by the Rivers Spean and Spey.

He next proceeded to discuss the theories of other geologists.

With regard to the theory that the parallel roads were formed by the *sea*, he adduced arguments to show, that this view was impossible, inasmuch as the "roads" should in that case have all stood at the same level; whereas, in the different glens, they stood at different levels. Moreover, it had been found, that old river courses existed, by which the water in Glen Gluoy flowed into the water in Glen Roy, and that the water in Glen Roy flowed through Glen Glaster into Loch Laggan,—a state of things utterly fatal to the marine theory.

With regard to the blockage of the lakes having been formed by *ice*, the following objections were stated:—

1*st*, The improbability that some of the glens were filled with water, whilst others were filled with ice, the temperature of those glens being all much the same, in consequence of nearly equal altitudes above the sea.

2*d*, The impossibility of getting a glacier to come to the exact spot, where the lakes stopped, to form barriers several miles long, so solid and permanent in structure, as to prevent the escape of the water from lakes above 300 feet deep.

The author concluded by referring to the numerous examples in the Lochaber district, of boulders perched on tops of hills, and of rocks smoothed and striated. These phenomena had been ascribed by some geologists to the action of land-ice. But, coupling with these high-perched boulders, the occurrence of kaims or eskars on the sides of the hills (above the parallel roads), and therefore formed before the Lake period, the author was inclined to ascribe these phenomena to one agent—viz., a sea loaded with ice, when the land was submerged, and to a strong current in the sea, from the north-west, which swept over the submerged land, and through such valleys as Glen Spean and Glen Roy. The lakes, he referred to the period when the land was rising out of the sea. Their beaches were formed on the marine detritus;—which also for a time dammed back the lake waters.

*Monday, 5th June* 1876.

D. STEVENSON, Esq., C.E., Vice-President, in the Chair.

The following Communications were read:—

1. Physical Observations in Northern Asia. By Professor G. Forbes.

2. On Parallel Motions. By the Rev. John Wilson, M.A., Bannockburn Academy. Communicated by Professor Kelland.

It has been well said that the transmission of force is an "essential condition in machinery." It is no less true that directness in transference is important; that the fewer links in the chain binding driver and follower together, the less likely is the machine to be put out of gear. There is no question here as to the comparative values of the different modes of conveying motion from a prime mover,—rolling contact, sliding contact, wrapping connectors, or linkwork. Each has its own excellencies; each its special advantages; and one is to be preferred to another only according to the nature of the work to be done.

I. *Watt's Parallel Motion.*—The general problem is the "commutation of circular with rectilinear motion." The importance of the question began to be felt soon after the introduction of the steam-engine; and Watt, in 1784, patented an invention which not only had the credit of being the earliest, but up to recent times, the most reliable and accurate parallel motion in existence. This system was a great advance on the huge chains and arches which were affixed to the working beam of the engine for the purpose of obtaining the desired motion; and it has proved to be sufficiently accurate for all practical purposes. The construction is simple, consisting of three bars: two, rotating round fixed centres, and connected at their other extremities to the third bar. A point in this bar, either within or without the points of junction, accord-

ing as the centres are on opposite sides, or on one side of the connecting rod, moves in a straight line. The simple explanation of the underlying principle is that the curvature in one direction is modified by the curvature in an opposite direction, due to the motion of the other bar. In both the original and the modified form of this three-bar motion there is a divergence from the straight line, which though inappreciable for small angles, becomes somewhat more apparent for larger ones.

The determination of the parallel point is given in the formula

$$QF : FP :: AP : BQ$$

In the figures 1 and 2, AP and BQ are the arms; PQ the con-

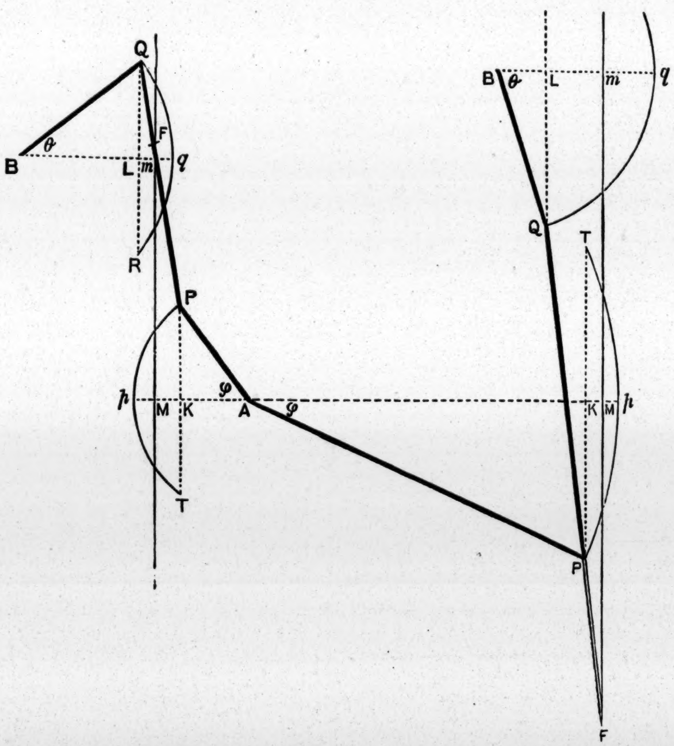



Fig. 1. Fig. 2.

necting link. Let the centres A and B be chosen so that the line $m$M shall bisect the versed sines of BQ and AP; the throw QR being equal to PT.

$$\begin{aligned} QF : FP &:: Lm : MK : Lq : Kp \\ &:: BQ\,(1-\cos\theta) : AP\,(1-\cos\varphi) \\ &:: BQ\cdot 2\sin^2\frac{\theta}{2} : AP\cdot 2\sin^2\frac{\varphi}{2} \\ &:: BQ\cdot\theta^2 \quad : AP\cdot\varphi^2 \quad .\;.\;.\;.\quad (1) \\ &:: BQ\cdot AP^2 \quad : AP\cdot BQ^2 \quad .\;.\;.\quad (2) \\ &:: AP \quad : BQ \end{aligned}$$

(1) For small angles, $\sin\frac{\theta}{2}=\frac{\theta}{2}$ nearly.

(2) Ultimately, $BQ \sin\theta = AP \sin\phi$, or, $BQ : AP :: \sin\phi : \sin\theta :: \phi : \theta$.

In connection with Watt's system we are led to consider the motion of the pantagraph, which has been a means of extending the parallel motion. Thus, while to the point F is attached the air-pump rod of an engine; by means of the pantagraph, whose property is that it describes similar curves on a smaller or larger scale, another point will trace another parallel line, and to it therefore may be fixed the end of the piston-rod. By superadding the parallelogram of bars to the Watt parallel motion, and making E,—the angle of the parallelogram—the connecting point of the piston rod, so as to concentrate the force; we have the point E describing a curve similar to F, when

$$QF : BQ :: PQ \text{ or } HE : BH. \quad \text{(Fig. 3.)}$$

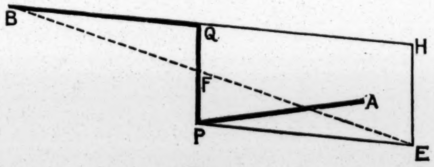



Fig. 3.

The problem of the pantagraph may be proved as follows. Let the initial position be at an angle of 90°, the arms lying along the axes of $x$ and $y$. (Fig. 4.)

Given the fulcrum A and the ratio AB : AC, to find $B_1$, the position which B the pencil assumes when the tracer C moves to $C_1$. When the position of $C_{11}$ is given, the angle $C_1D_1A$ is known, for $AD_1$ and $CD_1$ are constants. Join $BB_1$ and $CC_1$; $AB_1$ and $AC_1$ (which latter are not assumed to be coincident).

In the triangles $C_1D_1A$ and $B_1F_1A$, the angle $B_1F_1A$ is equal to the angle $C_1D_1A$; and the sides about these angles are propor-

tional, viz., $B_1F_1 : F_1A :: C_1D_1 : D_1A$. Therefore the triangles are equiangular (Euc. VI. : 6).

Wherefore the angle $F_1AB_1$ is equal to the angle $D_1AC_1$, that is they are identical; and $AB_1C_1$ is a straight line. Now in the triangle $ACC_1$, — $AB : AC :: AB_1 : AC_1$. Therefore $BB_1$ is parallel to $CC_1$; and $BB_1 : CC_1 :: AB : AC$, which is the given ratio.

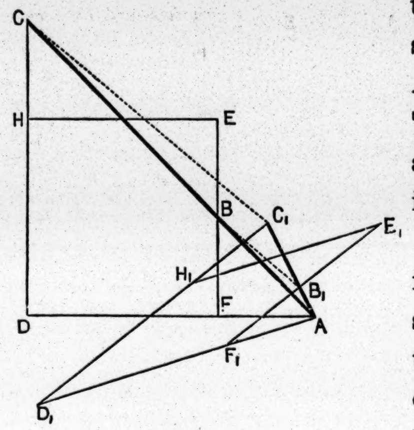


Fig. 4.

Wherefore the position of $B_1$ is found. And any other position such as $Cn$ will have its projection in similar ratio to the given one $AB : AC$ or $AF : AD$.

II. *The Reciprocator.*—About 1864 M. Peaucellier communicated to the Société Philomathique a paper anent a newly-discovered means of producing parallel motion; but, at the time, little notice was taken of the subject. When Lipkin, a pupil of Tchebicheff, rediscovered it a few years later, it awakened the attention of the most eminent mathematicians of the day; and now it promises to become a power in the field of higher analytical investigation. The marvellous extension of the problem is due chiefly to the research of Professor Sylvester, who was the first in this country to direct attention to it.

The fact of pure linkwork constituted the difference between Peaucellier's and all other attempts at parallel motion. These have been only approximate, or if exact, the slot has been called into action; while in the case before us the motion is entirely due to the grouping of links around fixed centres.

The system consists of seven links. (Fig. 5.) A cell or rhombus MPNC is jointed at two opposite angles to two links AM and AN, which are called connectors. To one of the remaining angles is attached a radial bar BP. Of the two fixed points, A is called the fulcrum; B, the pivot. The points C and P are the poles of the cell. The variable distances $\overline{CA}$ and $\overline{AP}$ are called the arms of the cell, while the difference between the squares of AM and MP is termed the modulus.

When A, the fulcrum, lies without the rhombus, the cell is called a positive cell; when it falls within, the cell is negative.

The following results can easily be verified :—

(1.) $\overline{APC}$ is a straight line.

(2.) $\overline{AP}$ . $\overline{AC}$ is a constant quantity (Euclid III. 36), $\overline{N}$ being at every moment the centre of a circle with a radius $\overline{NP}$ or $\overline{NC}$. This result is expressed by saying that the curves described by P and C are the inverse of one another. Now the inverse of a circle is generally another circle. If, then, one of the poles of the cell be made to revolve in a circle round B, the other pole will describe a circle. There is however an exception, for when the fulcrum is in the circumference of the circle described by P or C, the other pole describes a straight line.

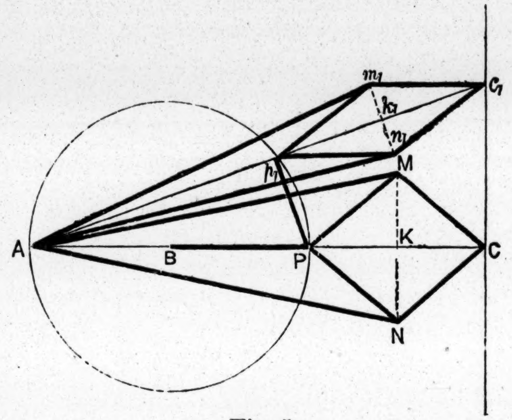


Fig. 5.

The following is the geometrical proof:—

Let the positions of P be $p_1$, $p_2$, $p_3$, &c., and the corresponding positions of C, $c_1$, $c_2$, $c_3$, &c.; and suppose that the initial position of the system is when AC lies along the axis of $x$, that is, in a straight line with the centres A and B. Join P and $p_1$; C and $c_1$. Join the corresponding pairs $Pp_2$, $Cc_2$, &c.

By Eucl. II.: 12. $AM^2 = MP^2 + AP^2 + 2AP \cdot PK$

$$Am^2 = mp^2 + Ap^2 + 2Ap \cdot pk$$

$$\therefore AP\,[AP + 2PK] = Ap\,[Ap + 2pk]$$

$$AP \cdot AC = Ap \cdot Ac.$$

$$\text{or } \frac{AP}{Ap} = \frac{Ac}{AC}$$

In the triangles $APp$ and $ACc$ the angle A is common; and the sides about it are proportional, viz.,

$$AP : Ap :: Ac : AC$$

Therefore the triangles are equiangular (Euc. VI. 6), wherefore the angle $c_1CA = Pp_1A$. But $Pp_1A$, the angle in a semicircle, is a right angle (Euc. III. 31), therefore $c_1CA$ is also a right angle.

With any other position $p_2$ of P, C will take up a corresponding one $c_2$, by joining which with C a right angle is formed. Wherefore the point C moves in a straight line at right angles to AB produced.

When the ratio subsisting between the length of the radial bar and the distance of the pivot from the fulcrum is not one of equality, the position of the parallel point is a circle, which is concave or convex with respect to the fulcrum, according as BP is greater or less than AB. The results, as determined by methods of analysis, show that the equation is the same for both the symmetrical and non-symmetrical forms of the cell; that is, for both the ordinary cell, where the pivot B in the initial position lies in a line with the fulcrum and the poles,—and for that form where the line passing the poles makes in the initial position a tangent to the circle described by the radial bar. Thus, if R were the centre of the circle described by C, its distance from the fulcrum A,

is $RA = \dfrac{AB\,[AM^2 - MP^2]}{BP^2 - AB^2}$; and the length of the radius

is $RC = \dfrac{BP\,[AM^2 - MP^2]}{BP^2 - AB^2}$.

The determination of the position of R, which is evident in the non-symmetrical form of the cell, is simply given in the ratio RC : RA :: BP : AB.

Thus, if $AM = 7$; $MP = 5$; $BP = 3$; and $AB = 1$

then $$RC = \frac{BP\,[AM^2 - MP^2]}{BP^2 - AB^2} = \frac{3(49 - 25)}{9 - 1} = 9$$

$$RA = \frac{AB}{BP} \cdot RC = 3$$

One form of the cell has given rise to a discussion of the question whether the parallel motion of Peaucellier is not simply a modification of the pantagraph. The resemblance between the two systems is not noticeable at first sight; and one would be inclined to deny any connection between them.

In this system the connectors are joined not at the opposite angles of the rhombus but at such points in the adjacent sides produced that at every moment they are parallel to the remaining sides (fig. 6). APC is a straight line. AM = CM since AM||LP. Let $\overline{LP}$ be produced to F, making $\overline{LF} = \overline{LM}$, and complete the rhombus $\overline{LFKM}$. The bars $\overline{AN}$, $\overline{CN}$, and $\overline{IP}$ can be removed without interfering with the motion of $\bar{C}$ and $\bar{P}$. Thus AM, MC, with LF, FK form a pantagraph, and for every position of $\bar{C}$, $\bar{P}$ takes another, equiangular and proportional.

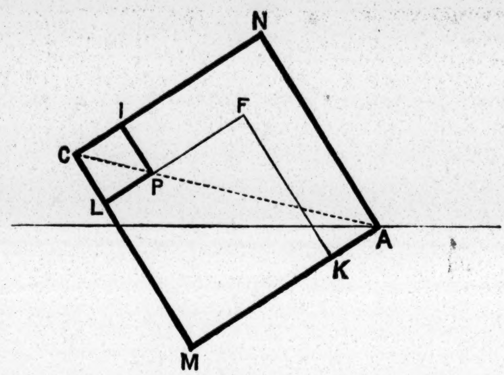


Fig. 6.

III. *The Gorgon Linkage.*—The parallel motion of Mr Scott Russell is exact, and constitutes a two-bar motion. From the fact that it was fitted by Mr Seaward to the engines of the "Gorgon," it may conveniently be called a Gorgon Linkage. A link AB is bisected in C, and at this point another link CD, equal to CA or CB is attached. D is the centre of revolution. If one end A of the link AB be guided along the axis of $x$, the motion of B is at every moment in the axis of $y$ from D.

For every position C is the centre of a circle $\overline{ADB}$, and ADB is the angle in a semi-circle, that is, a right angle. Hence B describes a straight line.

This system of links derives additional interest from the discovery of the Peaucellier cell, as by it the motion of the parallel point can be thrown in a direction at right angles to itself; that is, parallel to the line joining fulcrum and pivot; and this can be transferred to the line of centres by means of the pantagraph.

IV. *Hypocycloidal Parallel Motion.*—Another very interesting case of the problem of Parallel Motion is that produced by a hypocycloidal movement. When one circle is made to revolve on the concave circumference of another circle, any point in it describes a curve, which is called the hypotrochoid or hypocycloid. If the diameter of the revolving circle be equal to the radius of the circle

in which it revolves, the hypotrochoid becomes an ellipse; and if the point be *on* the circumference of the smaller circle, the ellipse degenerates into a straight line.

Let the circle BPC (fig. 7), diameter BC = AC, revolve in the circle ABM, radius AC. If the initial position of the describing circle be such that AC be its diameter along the axis of $y$, the point A will move along the diameter AM. When the circle has moved round to B, let P be the position of A; then AB = BP. Join BKC and KP. Let ACB = $\theta$; BKP = $\varphi$. The arc BP = AB $\therefore$ BK $\cdot\, \varphi$ = AC $\cdot\, \theta$ = 2 BK $\cdot\, \theta$. Hence $\varphi = 2\,\theta$. Hence P must lie on AC. (Euc I. 32).

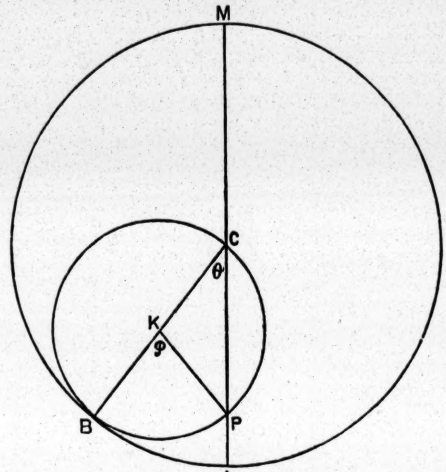


Fig. 7.

The difficulty of constructing, and the inconvenience experienced in using an arrangement in which the annular wheel comes into action, are so great that it is seldom employed. The following is a simple method of obtaining the hypocycloidal motion without requiring the annular wheel. On the arm ACB (fig. 8), which is made to revolve in a circle MLP round A, attach two toothed wheels LNF and FEM, in gear with each other and with a fixed wheel EOH, whose radius AE is double that of NLF. The centre wheel FEM is used merely for the purpose of reversing the motion of NFL, hence it is immaterial how many teeth it has; the only postulate of the problem being that LFN revolve on its axis at double the speed of the

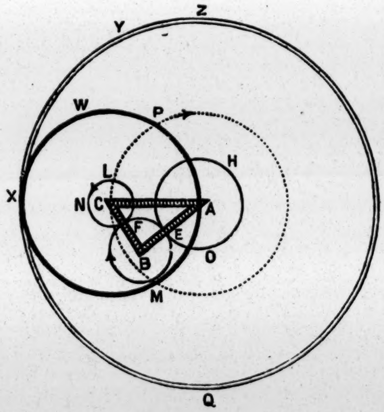


Fig. 8.

arm AC, and in a contrary direction. The number of teeth on HEO is twice the number on NLF. Hence NLF makes two revolutions round its axis, while the arm causes it to revolve round the fixed centre-circle HEO. The axis C is rigidly connected with a wheel XWP, revolving on the upper surface of the plate-arm ABC, whose circumference continually passes through A. Thus while the plate-arm ABC makes one revolution round A in the direction of the hands of a watch, XWPA, which is rigidly connected to the axis C of NLP and partakes of its motion, makes two revolutions in the opposite direction. Thus it revolves round an imaginary annular wheel XYZQ, whose diameter ZQ is double that of its own; and in virtue of hypocycloidal motion, if XWP be the initial position, the point A in the circumference of XWPA will move along ZQ.

In the annexed figure (fig. 9), the *modus operandi* of the machine is sketched. The system is analogous to the sun and planet wheel invented by Watt.

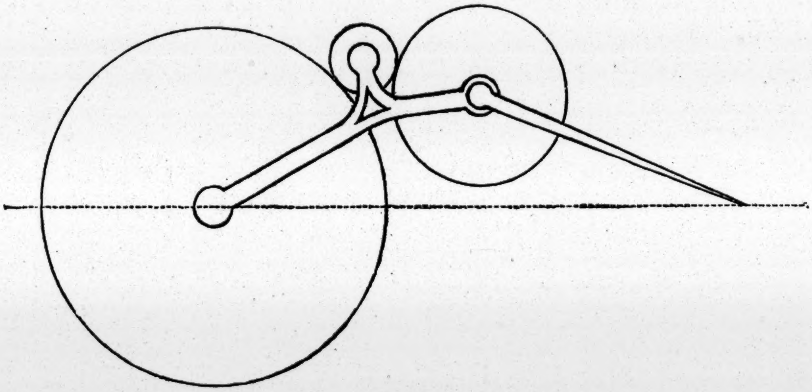


Fig. 9.

It would be going beyond the limits of this paper to direct attention to the two modifications of the Peaucellier cell, which have been called respectively the Quadratic-binomial Extractor, and the Conicograph. The introduction of these into the sphere of mathematical investigation has given several indications of valuable results to be obtained therefrom. For a brief outline of these, we refer the reader to the pamphlet of Professor Sylvester, who not only was the first in this country to direct attention to the general problem, but also had the credit of demonstrating the

higher vantage ground opened to the mathematician by means of Peaucellier's discovery. The extent of our own obligation to him is great.

Not less are we indebted to Professor Kelland, whom we have known both as a teacher and a friend. The valuable hints and suggestions he has given us on this subject we are glad to take this opportunity of acknowledging.

## 3. Laboratory Notes. By Professor Tait.

(*a.*) On the Passage of the Electric Current from Amalgamated Zinc to Zinc Sulphate Solution. By J. G. MacGregor, M.A.

(*b.*) On the Thermo-Electric Properties of Cobalt, &c. By Messrs Knott, MacGregor, and C. M. Smith.

(*c.*) Measurements of the Potentials required for Long Sparks of a Holtz Machine. By Messrs Macfarlane and Paton.

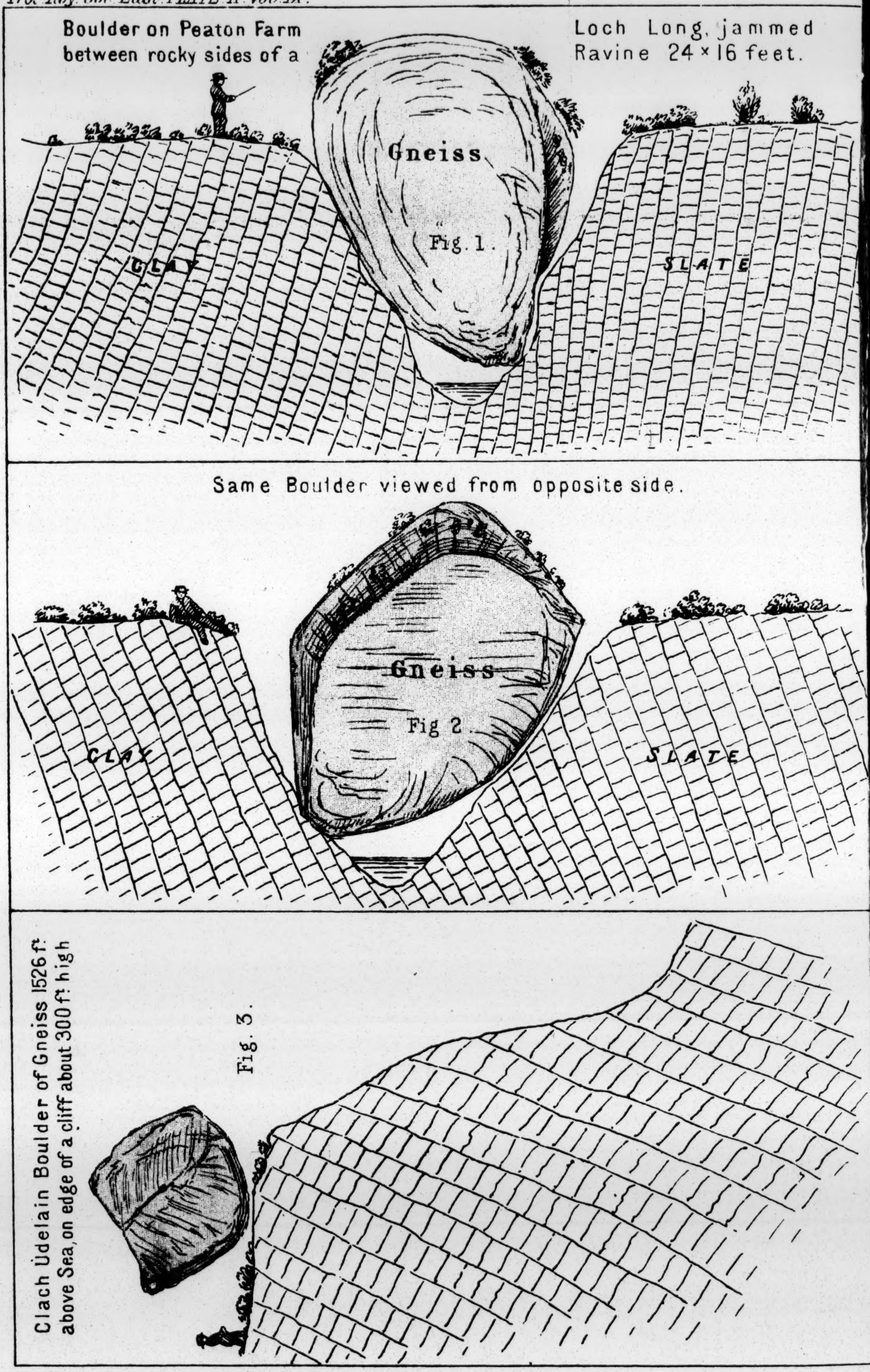
## 4. Note on Orthogonal Isothermal Surfaces. By Professor Tait.

## 5. Notice of some recent Atmospheric Phenomena. By Professor Tait.

## 6. Report by the Society's Boulder Committee. (Plates II. and III.)

Mr David Milne Home gave in the Third Report of the Society's Boulder Committee, from which the following are extracts:—In November 1875, on the invitation of Sir John Douglas of Glenfinnart, the Convener went to visit him at that place, to have an opportunity of examining several remarkable boulders reported to the Committee as situated in that part of Argyllshire.

1. On the east side of Lochlong, opposite to Ardentinny, there is the farm of Peaton. On this farm, a burn descends from a steepish hill which faces the north. A gneiss boulder lies in a gorge cut by the burn through rocks of clay slate. The boulder




Boulder on Peaton Farm Loch Long, jammed
between rocky sides of a Ravine 24 × 16 feet.

Fig. 1.

Same Boulder viewed from opposite side.

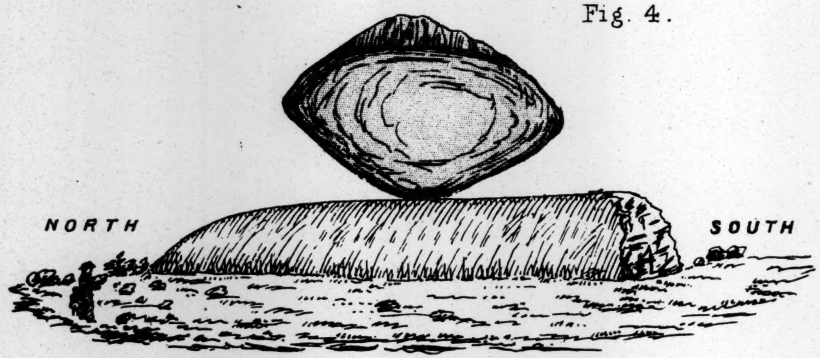
Fig 2.

Fig. 3.

Clach Ùdelain Boulder of Gneiss 1526 ft above Sea, on edge of a cliff about 300 ft high

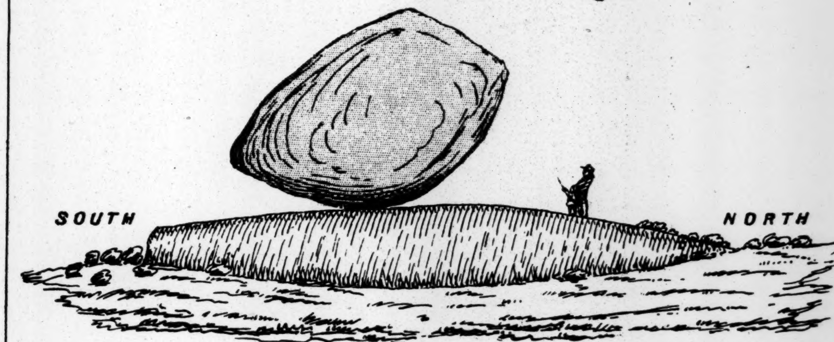
Boulder of Gneiss on a rock of Clay Slate Rock, smoothed from the North, as shown by rough parts facing the South.

Fig. 4.



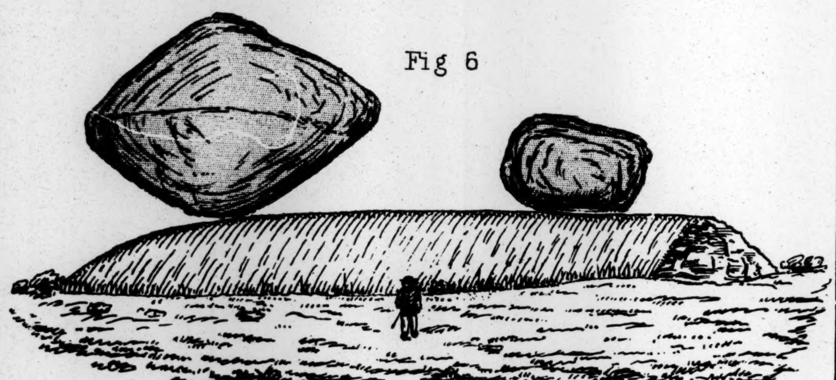

The Same Boulder viewed from opposite side. Local name "Giants putting stone."

Fig 5.



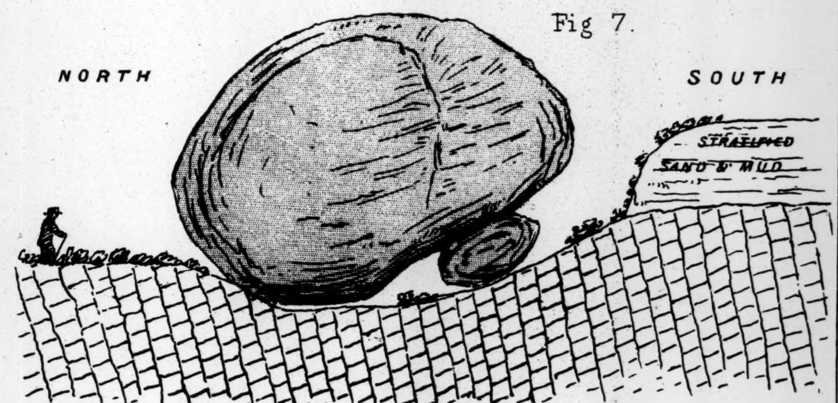

Gneiss Boulders North of Knap Farm House on Loch Long resting on smoothed clay slate rocks

Fig 6



Pulag Boulder, near Glenfinnart, on Loch Long, 824 feet above Sea. Its South side rests on a small Boulder

Fig 7.

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rests on the rocky sides of the burn, jammed in between the sides. The boulder has some local name like "Jenny Meullen," meaning "House on a knoll." The height of the boulder above the sea is about 226 feet. Its distance from the sea-beach is about three-quarters of a mile. The size of the boulder is about 24 × 18 × 12 feet. It is most probable that the boulder was transported across the loch from the north or north-west, and was arrested in its further progress southward by the hill, on the north side of which it stands.

Two sketches of this boulder are given on Plate II. figs. 1 and 2.

2. Between the site of this boulder and the sea-beach an old sea margin occurs at a height of about 45 feet above the sea (medium level). A number of boulders lie along the line of this sea margin. There is an old sea margin on the opposite or west side of the loch, at exactly the same height—viz., 45 feet.

3. Close to the beach in this part of Lochlong—*i.e.*, about 8 or 9 feet above high-water mark—at a place called "Letter," there lies another gneiss boulder, 12 × 8 × 8 feet. Its long diameter points N.W. by N.—viz., to Glenfinnart Valley.

4. Very near this boulder (about 100 yards to the north) the clay slate rocks have been ground down and smoothed. Their smoothed surfaces show numerous striæ pointing N., 2° or 3° W. (magnetic). The smooth surface dips towards the north at an angle of 3° or 4°.

5. On the hill above Carrick Castle, situated on Lochgoil, there is a boulder called "Clach udelain," or the "Stone nicely balanced." It is at a height of 1526 feet above the sea. This boulder is of gneiss, and lies on rocks of clay slate. It lies on a bare rock, the face of which slopes to N.N.E.—*i.e.*, towards Lochgoil. The boulder is within three or four yards of the edge of a precipitous rocky cliff, which goes vertically down about 500 or 600 feet. The block is of enormous size. Unfortunately the note taken of its dimensions has been lost. This boulder, from its position, could not have fallen from any hill. There is no hill near it from which it could have fallen.

A sketch of this boulder is given on Plate II. fig. 3.

6. The next boulder visited is about two miles to the eastward of the last-mentioned, and is within a quarter of a mile of Lochgoil,

near its junction with Lochlong. It is about 450 feet above the sea. It may be observed, that all the rocks in this district have their smooth faces towards the north, their rough faces towards the south. This boulder has received the name of the "Giant's Putting-Stone," from a legend which alleges that in former times there were giants who inhabited the district on both sides of Lochlong, and who were in the practice of amusing themselves by throwing these huge boulders across the loch. The rock on which it rests slopes gently N. by E. This rock presents a large surface, ground down and smoothed. The space of rock occupied by the superimposed boulder is only 18 inches by 12 inches. It would not be difficult for two men with strong levers to move the boulder from its narrow resting-place, in which case the boulder would probably roll down the steep hillside into the loch.

Two sketches of this boulder are given on Plate III. figs. 4 and 5.

7. To the north of Knap Farm-house, there is a small hill, on or very near the top of which eight or ten boulders are clustered. They suggest the idea that this hill has arrested or interrupted the body, whatever that body was, which transported the boulders, and caused them to be stranded here.

8. There is another hill lower down the valley of Knap (about 480 feet above the sea), the top of which consists of clay slate rocks, rounded and smoothed by some agent passing over from the north. It has received the popular name of the "Pig's Back." Several boulders lie on this ridge. The largest rest on a very small portion of rock.

A sketch of this ridge of rock, with boulders on it, is given on Plate III. fig. 6.

9. Pulag boulder is near the top of a hill to the west of Glenfinnart, about 824 feet above the sea. It is a large block of gneiss, about 7 feet high. There are many other smaller boulders lying near it. The large boulder is almost on the edge of a precipice which goes down at least 200 feet. It could not have been rolled or pushed to its present position. The levels of the district show the greatest openings towards the north—a circumstance which suggests that the boulder came from the north. Moreover, its south end rests on a smaller boulder, which seems to have stopped its progress further south.

A sketch of this boulder is given on Plate III. fig. 7.

10. Along various parts of the hills in this district where their highest ranges are seen against the sky, and at a height of about 2000 feet above the sea, boulders are discernible from a distance, lying on the ridges. It would be very desirable to obtain particular accounts of boulders at so high an elevation.

11. In the last Report of the Committee, notice was taken of a boulder in Ayrshire called the "Hunterston Boulder." Along the same coast, and especially on the property of Mr Alexander of Boydstone, several very large boulders may yet be seen. One, called the "Boydstone Rock or Stone," is situated about two miles north-west of Ardrossan. Some chips of the boulder, sent to the Convener in a letter, show that it is porphyritic. The rocks on this part of the coast are Old Red Sandstone. The boulder is in length about 19 feet, and in breadth about the same. It is partly buried in the mud of the shore. Its highest point is 9½ feet above the shore. It is said to contain 40 cubic yards above the shore line. It is supposed that the boulder is buried to the depth of 5 feet. The tide at high water leaves about 3 feet of the boulder visible. This boulder has inspired the poetic genius of an Ayrshire letter-carrier (Malcolm Kerr, post-messenger between Ardrossan and West Kilbride), who, through Mr Weir of Kirkhall, has sent to the Convener the following stirring address :—

*To the Great Boulder on the shore opposite to the lands of Boydstone, two miles north of Ardrossan, Ayrshire.*

"Can'st thou speak, old grey stone,  
Unto me ?  
List thou to the ocean's moan,  
I to thee :  
Music sweet ! Spirits string  
Wild ditties, as they cling  
To the big waves which swing  
Around thee.

Stranger ! whence didst thou come  
To this shore ?  
Art thou an Arctic crumb,  
Which of yore

On some huge iceberg side
From thy first home did glide,
A wanderer on the tide,
To this shore?

Many eyes with wonder,
Ages gone,
Looked on thee! What number
Yet unknown,
Will gaze with curious eye,
Seeking to know thy history,
And solve a hidden mystery,
Old grey stone."

Besides the boulder which inspired these verses, there are two others, also on Mr Alexander's property—one of them, as Mr Weir states, "even larger than the big stone at Brigurd, at Hunterston." This larger one it was proposed to split up for building purposes at Ardrossan; but Mr Alexander interfered, and saved it. There are many other boulders along the Ayrshire sea-coast, but none so large as the Boydstone boulder. A chip of one of these sent, shows that it is of gneiss, indicating a northern origin.

12. A report has been received of a gneiss boulder near Dunblane, on the property of Cromlix, belonging to the Hon. Captain Drummond. The length of the boulder is stated to be 17½ feet, its breadth (on an average) 10, its height about 5 feet. Its longer axis lies in a direction south-west and north-east. At its south end it dips into the ground at an angle of 45°. Its weight above ground is estimated at 34 tons. Its height above the sea is about 450 feet. It is about four miles south from the Grampians. The same reporter (Henry Wilkie, Ashfield Works, Dunblane) refers to a group of four boulders in the parish of Redgorton, at the west end of a gravel ridge on the farm of Bertha, the property of Murray Graham, Esq. of Murrayshall. Three of these Redgorton boulders are within an area of 30 yards. They are angular; flat on the top, and some of them square. They seem to be Silurian rocks. They are distant about twelve miles from the Grampians.

13. Notice has been received by the Convener from Mr Robertson, C.E., who is in charge of the Albert New Docks at Leith, that boulder clay was found in excavating for the docks beneath the shore line, to the depth of about 70 feet. The clay was full of

large blocks. Some of these were of sandstone, weighing 10 or 12 tons, and appeared to be of the same rock as that worked at Granton and Craigleith Quarries to the westward. Beneath the boulder clay, there was found what the engineers call a "running sand" lying over strata of shale and sandstone.

Before concluding, the Committee may advert to the circumstance that a part of the district comprehended in this Report was many years ago described by an eminent Scotch geologist—the late Charles M'Laren—and with reference to the very matters embraced in this report. Mr M'Laren read papers in this Society, in the years 1846–47, describing the boulders then existing on the shores of the Gairloch, and on the hills between that loch and Lochlong. Even then the destruction of boulders in that quarter had begun, being appropriated, as Mr M'Laren states, to building purposes; and probably by this time they have all been annihilated. Mr M'Laren in these papers described also the striation and smoothings of the rocks, which he found from the sea-shore up to the tops of the ridges, between the Gairloch and Lochlong, at heights of about 1000 feet above the sea. It is due alike to the memory of our Associate, and to the interests of geological science, to mention, that the boulders referred to by him, as found on the Gairloch, consisted of grey granite, of which he counted above a hundred, one-third of them exceeding 30 tons in weight; as also mica slate, which, though less numerous, had had among them blocks of 60 and 80 tons in weight. As the rocks of the Gairloch *in situ*, are of a more recent kind—viz. clay slate—Mr M'Laren justly inferred that the boulders were of northern origin. For those of granite, he pointed to Ben Cruachan, a mountain exceeding 3000 feet in height, situated to the N.N.W., and distant about thirty miles. The mica slate hills are also in the same direction, somewhat more distant. From his study of the boulders and other phenomena in this district, Mr M'Laren drew two important conclusions. One conclusion was, that the boulders must have been brought to the district from the parent mountains, across valleys and ranges of hills, on ice floating on a sea which stood from 1500 to 2000 feet above the present sea-level, and in which a strong current had prevailed from the N.W. This conclusion, it will be noticed, is confirmed by the facts specified in the

present Report, and also in former Reports by the Committee. The numerous instances given in these Reports of huge boulders shown by their composition to be of northern rocks, clustered frequently on the summits or peaks of hills, at heights of 1500 and 2000 feet above the present sea-level, seem to leave no doubt regarding the soundness of *that* conclusion to which Mr M'Laren had come to thirty years ago.

The other conclusion to which Mr M'Laren came, and which many good geologists of the present day hold, was this, that local glaciers had at one time existed in all those valleys. This he inferred from observing, that the striations on the rocks were all, as he thought, exactly parallel with the axis of the valley, and which here generally runs in a direction north-west and south-east; and also from discovering accumulations of gravel and clay in the form of elongated embankments—some across the valleys, others parallel with the valleys, reminding him of the lateral and terminal moraines of Switzerland.

As to the soundness of this second conclusion, or the correctness of the observations on which it was founded, the Committee give no opinion. None of the members of the Committee have visited the localities, and their function as a Committee has been chiefly to collect facts connected with the boulders. But it may not be out of place to record the fact, that another of our Associates, the late Robert Chambers, who had also given much attention to this branch of geological science, went to examine the districts referred to by Mr M'Laren, and expressed doubts regarding these last-mentioned views. In two papers read by him in this Society in December 1852, Chambers states, that on the rocky ridge between Lochlong and Holy Loch, at a height of 600 feet above the sea, the direction of the striations was not parallel with the valley, but "slanting over the hill;" and as the striations were not merely on the sides of the valleys, but on the tops of the hilly ranges dividing the valleys, he thought they were more probably due to a general glaciation of the country than to local glaciers. Chambers quotes also the opinion of Sir Roderick Murchison, "that there is no imaginable centre for the issue of glaciers of the ordinary kind down the Gairloch." On this point the Committee offer no opinion, and wish only to advert to the investigations of eminent Scotch geolo-

gists who in former days attempted a solution of these difficult questions. The Committee would venture to suggest a re-examination of the localities, as the fuller knowledge now possessed on these subjects may possibly throw a clearer light on the phenomena.

The Committee wish only to add, that if any geologists, whether fellows of this Society or not, happen, in the course of their rambles through the country, to fall in with or hear of any boulders remarkable for size, position, or composition, not yet mentioned in the Committee's Reports, the Convener will be happy to receive a notice of them.

After some conversation, in which Mr Ferguson of Kinmundy, Dr Bryce, the Rev. T. Brown, and others took part, the Report was adopted, and the proceedings terminated.

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